

some other force than that of gravity due to the assumed gradient. But it may be shown by the other method of considering the problem, by assuming a motion towards the pole of one mile per day, that the forces which overcome the resistances in both directions are about of the same order. This very great difference in the results obtained from the two methods of considering the problem, indicates that there is some great fallacy somewhere which needs looking after.

Mr. Croll is misled by adopting the erroneous principle that the amount of work performed by gravity upon a falling or descending mass is in all cases expressed by the mass multiplied into the height through which it descends, or that the foot-pound is a unit of work. The amount of work required to give velocity to any body, or overcome any kind of frictional resistance, is expressed by the intensity of the effort, regarded as constant, multiplied into the time of action. The intensity of the effort of any force, as of gravity, is explained by the mass multiplied into the velocity which such force can produce in a unit of time. If we, therefore, put  $g$ ,  $m$ ,  $t$ , and  $v$  for the force, the mass, the time, and the velocity respectively, we shall have, putting  $W$  for the amount of work performed by gravity,

$$(1) \quad W = mgt = mv,$$

that is, the amount of work performed is expressed by the momentum. Now this amount of work is stored away in the moving body, and remains until it is used in overcoming resistance of some kind, as friction or the inertia of other bodies, and  $W$  is exactly the expression of the working power which has been communicated to it. But if the moving body has been subject to resistances, as of friction, during the time  $t$ , then we shall have

$$(2) \quad W = mv + ft$$

putting  $f$  for the coefficient of friction, and supposing it to be constant. In this case  $mv$  expresses the amount of work left which has not been expended in overcoming the friction during the time  $t$ , and of course in this case we cannot get all the work back again which has been expended, at least mechanically.

If we now suppose a body to fall in a vacuum through the space  $s$ , if the amount of work performed by gravity upon it, and the working power communicated to it, is expressed by the mass multiplied into the height through which it has fallen, we shall also have

$$(3) \quad W = ms = \frac{1}{2}mgt^2 = \frac{1}{2}mv.$$

Hence, comparing the preceding expressions of  $W$ , we have  $mv = \frac{1}{2}mv$ , which is impossible; and therefore, if the former expression of  $W$  is correct, the latter is not.

Again, illustrating by a special case, if we suppose a body, of which the momentum is  $mv$ , to move upon a level plane without friction, and the plane to curve up in the direction of motion, and also suppose another body with half the mass and double the velocity, of which the momentum is  $\frac{1}{2}m \times 2v = mv$ ,  $m$  and  $v$  being the mass and velocity of the first body, it must be admitted that the amount of labour expended in giving both bodies the momentum  $mv$  is exactly the same, in the latter case the intensity of the effort being half as great, but the time of action twice as long; but the momentum will carry the latter up the slope to a height four times greater than that of the former, and after descending again to the plane, both will have the same momentum, and the same amount of labour would be required to bring each to rest, and consequently both have the same power of doing work. But the mass of the latter multiplied into the height through which it has descended is double that of the former, and hence these products do not express the power of doing work which gravity has communicated to them.

In the case in which the descending body is resisted by friction, we have seen (2) that neither the mass multiplied into the height of descent, nor the momentum  $mv$  which the body has on arriving at the level plane, expresses the whole action of gravity, and the resistance  $fv$  may be so small as not to affect sensibly the amount of work expressed by  $mv$ , or it may be so great that  $mv$  may be neglected, in comparison with  $fv$ . The value of  $fv$  may also be so great that the value which  $mv$  would have (1) in the case of a free body falling through the space  $s$ , would be almost infinitely small in comparison with the whole expression of  $W$  (2). In the various cases, therefore, which may be supposed, in which friction may be either very small or very great, so that in the former case the effect of the resistance might be scarcely sensible, and in the latter it might take the action of gravity a long time to drag the body down through the space which it has to descend, we cannot suppose that in all these cases the whole

action of gravity is expressed by the same number of foot-pounds, supposed to be units of work.

If, therefore, Mr. Croll's pound of water were moved from the equator to the parallel of  $60^\circ$  by the action of gravity without any resistance, the momentum which it would have on arriving there would express the work done by gravity upon it, and not the six foot-pounds, and the work would be done in a very short time in comparison with the time in the real case of nature; but when it is dragged down there through all the resistance which it suffers, at the rate of a mile per day, as we have supposed, the amount of labour which gravity performs is very many times greater than that expressed by the momentum which the pound of water would have on arriving there without resistance; and with regard to the six foot-pounds, we have seen that the work is no more comparable to them than a surface is to a solid.

Again, if we suppose the gradient upon which gravity acts to be only one foot in the distance instead of six, and the resistance to the pound of water to be as the velocity, then the water would move with only one-sixth of the velocity in the other case, and the water would be six times as long in reaching the parallel of  $60^\circ$ , but the energy of the action of gravity would be only one-sixth as much, and hence the work would be the same, being carried on six times as long in the latter case with one-sixth of the energy. But then the same work would be represented by one foot-pound, if that is a true unit of the work instead of six. In this case also the deflecting force eastward would be only one-sixth as much, but the time being six times as long, the same amount of work would be done, and this would be sensibly the same as that which would be required to give the pound of water a velocity of about 760 miles, as Mr. Croll has it, but really double that amount.

I am well aware that in the action of machines in which force is balanced against force, and consequently the times of action are the same, the amount of work may be expressed by the forces multiplied into the spaces through which they act; but in all cases in which the times differ, the amount of work cannot be expressed by any unit into which the element of time does not enter. The amount of work required to produce a velocity of 760 miles per hour is a function of the time, and proportional to the time where the force is constant, and cannot be measured by foot-pounds.

With regard to the argument based upon M. Dubuat's experiment, the matter briefly and fairly stated stands thus: according to the experiment, water will not flow unless acted upon by a force equivalent to that of gravity upon a given gradient, which makes the force required to move it about fifteen times greater than the horizontal component of the moon's disturbing force which produces the tides. But this force of the moon does move the water of the ocean, and therefore M. Dubuat's experiment is not applicable to water of great depth as of the ocean, and the argument fails. It is true, as Mr. Croll states, that the two cases of motion are somewhat different; in the case of the tides the water from top to bottom flows in the same direction, while in the other the upper and lower strata flow in contrary directions, and the resistances to the lower motions are no doubt greater. Six of the nine feet therefore of Mr. Croll's gradient should probably be given to the lower currents, and only three to the upper ones. But Mr. Croll admits that in the case of the tides a gradient of one inch is sufficient to move the water. A gradient of three feet only, therefore, ought to be sufficient to move the upper half, which would correspond somewhat to the case of tides in an ocean of half the depth. The observations of Col. Graham show that the water of Lake Michigan, about 700 feet deep, readily yields to the moon's disturbing force which causes a tide at Chicago with a range of nearly two inches.

Cambridge, Mass., Sept. 7

WM. FERREL

### Spectral Nomenclature

It seems almost absurd that a subject of such interest, and, as I think, importance, as that of Spectral Nomenclature should be discussed from opposite sides of the globe alone; so it may be hoped that it will not have been allowed to end with Prof. Young's remarks upon the one or two points in which he differs from me, but will have been taken up by others at a less distance. There is a great deal more to be said about it; but probably I should not have troubled you again just yet but for the obligation I feel to disclaim credit which he gives me for what is not mine.

Doubtless it is not necessary, but the obligation is the same; and a mistake into which Prof. Young has fallen is open to others.

The map which he wants—to be based on inverse wave-length or rapidity of vibration or pitch—is, I believe, in course of construction by Dr. Huggins, to whom, and not to me, is due, I think, the first idea and the proposal. Prof. Young has probably associated my name with it through a lecture delivered by my brother at Glasgow, in 1869, in which it was advocated.

I also wish to acknowledge that I was not aware of the fact which has now been so decidedly stated, that the coronal green line is certainly *practically* identical in position with 1474 (K), as tested by direct comparison. Indeed, I was ignorant that such a comparison was possible, having supposed that the line in question was only visible during eclipse. I ought, of course, to have referred to Prof. Young's "Preliminary Catalogue," and probably should have done so had I been in a house instead of in a tent, a few score of miles from the nearest station. But in truth it did not occur to me that there could be any certainty about the position of a line which, as a coronal line, had never been fixed by measurement. I may now venture to ask, What guarantee was there that No. 31 of the "Preliminary Catalogue" was "the coronal line," anterior to the Dodabetta measurement? I do not question it now, but I should like to know if the presumptive identity is supported by any characteristic difference between that line and those which are presumably due to the chromosphere. There is still a link wanting.

However, admitting the identity, and therefore the accuracy of the assigned position, we may still believe what Prof. Young says he would be glad to see proved, that "the apparent coincidence (with the iron line) is merely a very close juxtaposition." More than this: even were a very much higher dispersive power to show no resolution of the identity, should we not still be in nearly the same position as to any inference to be drawn therefrom? Evidence of physical relation between metals which present one or more lines common to both spectra may, indeed, eventually be shown (by the improbability of so frequent an accidental concurrence) to amount to proof. But this must be a prior step. To conceive it taken, and then to apply the like reasoning by analogy to the case of the single coronal line tallying with an iron line, seems to me speculation of the second order. Undoubtedly it would be matter for congratulation to be relieved from the liability to temptation of this kind by definite disproof. In the meantime, I cannot but regret that Prof. Young has half neutralised the good of a plain disavowal of belief in the ferrous interpretation of the coronal green line, by hazarding the query whether it may not "turn out" to be quasi-ferrous.

I am sorry—to return to the subject of nomenclature—that your respondent does not agree to my objection to "D<sub>3</sub>." Is it not plain that such a designation is haphazard? The association of idea is with D<sub>1</sub> due to sodium, instead of with the origin or source of the line. It tells nothing beyond the position, roughly, in the spectrum, by reference to a position which we happen to be familiar with, but with the occupant of which it has no connection otherwise. The name, in short, has no foundation in principle; and that, I apprehend, is a lack of the first requisite in a scientific name.

The objection to Greek alphabet letters is of a different character, but not less easily answered. It is very true that, through the exertions of Prof. Young and others, "the whole Greek alphabet would not suffice to name one in three of the lines" already known; but it would nevertheless suffice (as in the somewhat analogous case of the stars) "to express as many as the memory would require to hold." There is ample precedent. The principal lines of the elements, like the principal stars of constellations, are known to some extent by Greek letters; and as for the difficulty in respect of order, there was a time when the "lucid" stars, though very many in number, and having no very clear claims to precedence, were only known individually by personal names. Yet no sooner did a Bayer rank and name them according to apparent brilliancy, by Greek letters (to say nothing of the Roman), than the advantage of a fixed nomenclature was recognised and his work accepted; although observation must have shown that the assigned order was not always strictly correct. So would it happen now if, the lines having been lettered, further knowledge should show that the established precedence was not quite all that could be wished. The evil of slight incorrectness of this kind would be felt to be trifling compared with that which would result from an unsettling of a nomenclature established solely for convenience and involving no theory.

This would not prevent nor conflict with, neither would it render unnecessary, a far more extended tabulation depending on refrangibility. On the contrary, the want of such a classification and means of indication would be felt as soon as precise tabulation should come to be undertaken. In Kirchhoff's solar chart we have, graphically, something like what is wanted in a much more general, numerical, and tabular form—an example of a catalogue of lines. Charts are very useful, but not most handy; and they are not susceptible of such ready improvement and extension. The accumulation of results of spectroscopic research must sooner or later take the form of a catalogue of lines, from all sources, arranged in order of refrangibility; designating individuals (for special reference) where possible, according to their parent element or compound, their physical source, their cosmical habitat, or other characteristic and distinguishing indication, implied under the system of nomenclature which may be adopted; upon which would follow such details as to character (including intensity, width, definition, complexity, variability, &c. &c.) as present knowledge or future research may represent as suitable material for incorporation.

So long as spectroscopic analysis is content to remain in its earliest stages—and it must be allowed to be still in its infancy, though a giant from its birth—the student and experimentalist may to a very considerable extent learn by heart or by practice such spectra as he needs; but this can never suffice for all purposes. Accumulation is continually going on, and products must be stored. Let that be once acknowledged and the task attempted, and it must follow that, no matter how rigorous and precise may be the system of tabulation, there will be not only room, not only gain, but a positive necessity for an intelligible use of that kind of descriptive indication which is only to be found in scientific classification and nomenclature.

A general catalogue such as I contemplate would command, if compiled with even moderate knowledge and care, a very general acceptance. Unquestionably it would be extended, modified, improved upon, by subsequent work; but, so far as nomenclature is concerned, it would probably undergo but slight alteration—the less the better. It would form a basis on which any number of special catalogues might rest, without interfering with its permanence as a catalogue of reference.

I do not pretend to say that the task is a simple one; quite the reverse. But, then, all the more honour to whoever accomplishes it.

J. HERSCHEL

Bangalore, July 29

#### Jeremiah Horrox

I OBSERVED in a number of NATURE some three weeks ago an inquiry relative to J. Horrox, the astronomer. My wife is descended from Horrox, and I knew that one of her friends had his life, but have not been able to find it till now. The book has just been sent to me here.

The Rev. Jeremiah Horrox was born in Toxteth Park, near Liverpool, in 1619, and died in 1641, aged 22.

The life is by the Rev. A. B. Wharton, published by Wertheim, Macintosh, and Hunt, 24, Paternoster Row, 1859, and includes a translation of his discourse on the Transit of Venus.

HENRY HOLIDAY

Muncaster Castle, Ravensglass, Carnforth, Sept. 20

#### Millions of Millions

WHY do not Messrs. Ranyard and Co. adopt the late Benjamin Gompertz's most convenient notation of prefixing a circle to the first significant figure, or suffixing a circle to the last significant figure having therein a digit for the number of zeros employed?

Thus:                    ⑤718 is 50000718  
And                      718⑥ is 718000000

S. M. DRACH

74, Offord Road, N., Sept. 17

#### Analogy of Colour and Music

IN NATURE, No. 150, p. 393, a letter from Mr. G. C. Thompson is headed "Correlation of Colour and Music." As this letter refers to a paper of mine published some time ago, permit me just to say that Mr. Justice Grove has in your journal objected to the use of the word "correlation" employed in this sense. Entirely coinciding with the opinion of the eminent parent of this